authors have laid themselves open to criticism. Although we have delineations, and accurate ones too, of almost every known morbid alteration, yet so little *system* has been observed in their arrangement, so many dissimilar diseases brought at one and the same time, before the eye of the student, that he becomes lost in the maze of *disease*, and but too often finds himself unable, after all his labour, to distinguish one morbid lesion from another! Dr. C. in order to avoid this confusion, treats of but one elementary lesion or morbid alteration at a time, and traces it in its several changes in various organs, for instance the first fasciculus, (the only one as yet published,) is dedicated to the delineation and description of tubercle; its location, colour, consistence, composition, origin, progress, and effects upon the organ in which it may arise, are all noticed, and with a fidelity to the facts developed by dissection, and a searching scrutiny into their relative value, that reflects the highest credit on Dr. C.'s talents, industry, and capacity for the execution of the truly arduous task which he has undertaken. Four coloured lithographic plates, each containing four or five drawings, exhibiting tubercle in several different organs, accompany this number. These are executed in a masterly style, and convey, generally speaking, correct and highly interesting views of the morbid appearances produced by this almost inscrutable disease. This work deserves, and will no doubt receive the patronage of the profession, and we do not hesitate to say, that in continuing it, as he has begun, Dr. C. will confer an everlasting benefit upon the community at large, and establish for himself the enviable name of a skilful and faithful pathologist.

T. D. M.

XXI. *Elements of Chemical Philosophy on the Basis of Reid, Comprising the Rudiments of that Science, and the Requisite Experimental Illustrations, with plates and Diagrams.* By Thomas D. Mitchell, M. D. Professor of Chemistry and Pharmacy in the Medical College of Ohio, &c.

Dr. Mitchell has well remarked, in his preface, that the offer of an additional treatise on elementary chemistry, at this time, may seem a work of supererogation, and yet the reasons which he assigns for encountering critiscism on this head, are such as almost every lecturer on the subject, will pronounce to be valid. With the elaborate treatise of Professor Silliman, the American reprint of the last edition of Dr. Henry's copious work, and the more condensed and convenient work of Dr. Turner, which has already gone through four editions in this country, before us, we should be prepared to scrutinize closely a new compilation, were it not that the kindly feelings excited by an attempt to diffuse useful information through the western parts of our country, check, at the outset, such an examination as the nature of the case would otherwise suggest. In truth it requires all the effort of those feelings to examine with impartiality a work on the basis of the system, which we have conceived to be least adapted to students, and least complete of any of those which have appeared more recently in one language, and which always struck us as being a "practical chemistry"* only in name. The American editor has however much improved the work for the use of the student, and has made both additions and subtractions of a useful kind. The whole of the first part of the work Dr.

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* Elements of Practical Chemistry, &c. By David B. Reid, &c. &c.

Mitchell is directly responsible for, as in part a compilation, and in part original. This contains a general account of chemical affinity, treats of light, heat, electricity, &c. and gives a brief account of the nomenclature in use in England and this country, and devotes a chapter to the discussion of specific gravity.

The treatment of the subject of affinity is plain, and though elementary, is, upon the whole satisfactory. Some of the details, however, we conceive to be erroneous, such as that the attraction of gravitation is the same force with cohesion, a force acting at *sensible* distances, and decreasing in the inverse ratio of the square of that distance, the same as a force acting only at *insensible* distances, and therefore decreasing in a high ratio of the distance; that ammonia is the only *possible* combination of nitrogen and hydrogen; that the ratio of one to one and a half in combinations stated in equivalents, will disappear in compounds in the progress of modern chemistry. It may be that we lay too great stress upon the necessity of giving the student clear views of the laws of definite proportions, and that little more than a page is required to state all that should be known on the subject. On the necessity for an *accurate* expression of those laws there cannot be two opinions. The *one* which our author gives is:—

> "A striking feature in this view of combination, is that among the different uniting properties of the same bodies, there is an intimate and uniform relation, whereby the quantity of one of the two bodies which enter into a second combination, is a multiple, by a whole number of the quantity contained in the first combination."

The exceptions to such a statement are too numerous in the actual state of science to admit of its use, even if we adopt entirely the system of rejecting small fractional numbers. The atomic theory affords a strong ground for the rejection of the idea, based upon the reciprocal nature of the affinities of the particles of bodies. The cautiously accurate expression of this law, by the editor of the American edition of Turner's Chemistry, stands in such bold relief to the above, as to require merely to be quoted as the best commentary upon it.

> "When one body B, combines in two or more proportions with another body A, the numbers representing the quantities of B, combined with the same quantity of A, are multiples by a whole number of some particular number; that is, contain some number a number of times without a remainder. The number so contained may be the same as the number representing the lowest proportion, or it may be different."

Information upon subjects of physics, (heat, light, &c.) is generally given in considerable detail in chemical works; we should not disapprove a contrary course, namely, one which would confine itself to such discussions only as are necessary for the elucidation of chemistry, properly so called, but we hold that in this latter case peculiar caution is required as to the entire accuracy of the information conveyed: a student thoroughly imbued with the subjects under discussion, may be able to allow for looseness of expression, or to correct inaccuracies of detail, but when the information is only general, he has no means of so doing. These remarks have a direct bearing upon the portion of Dr. Mitchell's work now under consideration, as the marginal queries which we have had occasion to make would testify; our limits must prevent our bringing forward many examples. The much dwelt-upon experiment with the concave mirrors to exhibit the reflexion of heat, is an instance in point.

> "Substitute a lump of ice for the heated iron ball, and very soon the thermometer in the other focus will be affected, and its fluid will fall. Now, say some, this is proof that cold is a positive agent, and not the mere abstraction of heat. They contend, that the frigorific particles pass from the ice to the thermometer, and act on it, by a positive power. This, however, is a mistake: remove the ice from the focal point to a place midway between the mirrors, and the thermometer will not be affected; whereas, on the supposition of a positive frigorific agency, the reverse should occur."

Just such an experiment could be made, the mirrors not being near to each other, with the heated ball, and just the same reasoning used to show that heat is not a positive agent. But the deductions would be alike unwarranted by the experiment. Those who suppose *cold* to be a positive agent, trace it from the cold body to the adjacent mirror, from this by reflexion to the second mirror, by which it is converged upon the thermometer in the focus; and the experiment stated, plainly does not prove that such is not the case, neither would a similar one show that heat did not pursue the course just traced for *cold.* The reasoning does not touch the point, and the last and lately deceased advocate of the doctrine of *cold pulses* would have triumphed in such an examination of his theory.

We pass over the description of the mode of righting the walls of the Conservatoire des Arts, which if not accurate, answers all the purpose, and that of the scale of Wedgwood's pyrometer, the instrument having ceased to be one relied on for philosophical purposes. The following definition of conducting power is not so to be treated as a matter of indifference.

> "All heated bodies seem to have a disposition to send out their caloric to other bodies; or rather this tendency to escape, seems to be the characteristic of caloric itself, and hence we have the term *conducting power*, and this property is exceedingly unequal in different substances, &c."

Again, the experiment given to show that water does *not* conduct heat, will when performed with the requisite nicety, not be attended with the results stated, and will show either that the vessel containing the water has *conducted* heat downwards, and then imparted it to the water, or that the liquid itself is a conductor, a result decisively proved by well-known experiments. But these few remarks must suffice for the chapter now before us.

The chapter on the nomenclature of chemistry, without going into detail, gives a view of that in use in England and this country, and which the professor has deviated from in more than one instance which we may notice in the sequel. For the early date at which the philosophical views of Dr. Mitchell[*] in relation to the non-existence of any principle which could exclusively be allowed to be *the* acidifying principle, he deserves credit.

The following directions are given in a brief exposition of the subject of specific gravity, for ascertaining the specific gravity of a body which is lighter, bulk for bulk, than water.

> "Attach to the light body, by means of a fine thread, another body, sufficiently heavy to sink both in water; having previously taken the weight of the heavier solid in water, and of each in air, we are next to weigh them jointly in water, and from the difference between their weight in water and in air, we

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[*] New York Med. Repos. 1812-13.

must subtract the difference between the weight of the heavy solid in air, and its weight in water. The remainder will indicate the weight of a quantity of water equal in bulk to the light body, and we can thus find its specific gravity in the way directed above."

The directions include one operation of weighing, and two of calculation, which are unnecessary; viz. weighing the heavy body in air, adding that weight to that of the light body, and then taking it from the sum.

We have now arrived at the "basis," more properly chemical, though avowedly less original than the portion which we have cursorily examined. The author has taken pains in most cases to assert the rights of his American brethren, and to notice their experiments particularly. Thus, the early experiments of Dr. Woodhouse on carbonic oxide, on the inflammable mixture resulting from the calcination of carbonate of potassa with soot are given; the oxy-hydrogen blow-pipe is attributed to its real inventor, and its merits properly enforced; the excellent method of preparing nitrogen given by Professor Emmett is referred to; the self-regulating reservoir for hydrogen of Dr. Hare is spoken of; the experiments of Dr. Bridges on the residuum from the preparation of oxygen from nitre are given, &c.

The details of operations and cuts of apparatus given by Dr. Reid are omitted in this edition of his work, but the useful diagrams for exhibiting combinations by weight and volume, and for indicating the results of complicated reäctions, which by the by we consider the best part of his work, are retained in the American edition. In the order of substances, oxygen and hydrogen are given first, their compounds are next noticed, then follows nitrogen, its compounds with oxygen, sulphur and its compounds with oxygen and hydrogen, selen*ium* and its compounds, phosphorus and compounds, carbon, boron and compounds, chlorine, iodine, &c. then the metals in orders, *ammonium* being included in the list; a brief chapter on animal chemistry, and one on analysis close the work. Vegetable substances are treated early in the stage of the work under the head of compounds of carbon and hydrogen. It would be obviously impossible to carry our readers over this ground in reasonable limits, and as the criticisms would bear on Dr. Reid oftener than upon Dr. Mitchell, the task is not one for which our readers would thank us. A few points in the original matter seem to require notice, one of them is the discussion of the question, whether atmospheric air is a chemical compound of nitrogen and oxygen, or a mere mixture of those gases. We quote—

"And here it may be affirmed, that in the operation of chemical attraction between any two bodies, an entire change of properties follows. What then are the properties of oxygen and nitrogen in a separate state, and what are the peculiarities of atmospheric air? If none of the essential and characteristic qualities of nitrogen are found in atmospheric air, then it follows that a chemical action has ensued, between it and oxygen, in the formation of common air. The leading feature of nitrogen is its unfitness for respiration and combustion; animals die when subjected to its influence, and lighted tapers are instantly extinguished. But atmospheric air is the grand element of life; and equally indispensable in the maintenance of ordinary combustion."

"There are also dissimilarities between oxygen gas and common air. The former is too stimulating to be constantly inhaled, while the latter is precisely adapted to the condition of the lungs in a healthy state. They differ moreover in specific gravity, and in their powers to maintain combustion. It is plain to the most cursory observer, that the distinctive features of nitrogen and oxygen do not exist in atmospheric air."

"But it is said, that nitrogen and oxygen will be found detached from each other, in a tube which has been filled with common air, and closed perfectly tight, after some months have elapsed; and so also will mercury be separated from the oxygen with which it was combined in the form of oxyde, after the lapse of a given period; and yet no one, for this reason, denies that oxydes are chemical compounds."

The same train of argument might be used to show that any mixture of solids, liquids, or gases, was a chemical combination, for example, of sand and sugar, of carbonic acid and hydrogen. What can be meant at the present date by saying that nitrogen and oxygen confined in a tube will separate from each other? Have the experiments of Dalton, of Mitchell and Graham, on the diffusion of gases through each other gone for nothing? Or has not the author subjected himself to a just stricture for enforcing by such arguments, and by such statements, a theory, to which the circumstance, that it was supported by him in 1813, lends undue weight?

"These," says our author, "are the views that I have entertained on this subject for twenty years; for a fuller account of them, the reader is referred to the *Memoirs of the Columbian Society*, published at Philadelphia in the year 1813."

There are few opinions in a rapidly advancing science, which it would not be well to revise under the lights of new facts, oftener than once in twenty years.*

We will hastily notice a few of the marginal references made in looking over the remaining portion of the work.

An original remark on the chapter on nitric acid contains a criticism of Liebig's test for that acid by the sulphate of indigo, in the use of which we will promise our author success if he will substitute *muriatic* acid in lieu of the sulphuric acid which he adds to the mixture of the acid to be tested with the diluted sulphate of indigo.

The *oxygenated* nitric acid would have been well left out, notwithstanding its insertion by Dr. Reid; the discovery of the deutoxide of hydrogen prevents the necessity of our entrance into the labyrinth of highly oxygenated substances which the researches of Thenard, precedent to that discovery, had presented to him. In the compounds of carbon and hydrogen the remarkable fact, which bears so strongly on the philosophy of chemistry, is overlooked; namely, that their atomic constitution cannot with propriety be stated so as to reduce the numbers to their prime or equivalent ratios; thus, it seems that one atom of olefiant gas is not composed of one atom of carbon and one of hydrogen, but actually of two atoms of carbon and two of hydrogen. Admitting the superolefiant gas of Mr. Dalton, which has not been separated from mixture with the other carburets of hydrogen, both that gas and one discovered and examined by Mr. Faraday consist if the prime ratios of the equivalents be taken, of one equivalent of carbon and one of hydrogen, but are, more consistently, stated the former to consist of three atoms of carbon and three of hydrogen, forming one atom of the gas, the latter of four atoms of carbon and four of hydrogen forming the compound atom.

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* In passing we object to the derivation of *eudiometer*, "literally, a *measurer of air*," and to that of ammonia, "from *Jupiter ammon*."

The experiments of Mr. J. F. W. Johnston have satisfactorily shown that the black residuum from the preparation of cyanogen is *not* carbon, but consists of carbon and nitrogen, showing the necessity for the reëxamination of so many of what may be called the *traditional* statements of the science. Our author is here a little behind the *year*, as well as in some other matters, for example, in the statements relating to protoxide of chlorine which we conceive to have been satisfactorily proved, by the experiments of Soubeiran, to have no existence.

But we grow tedious with desultory notices, and will close by noting two points, in relation to which the task of fault-finding is not imposed. The first of these is an interesting statement of the assertions of Lavoisier and others, of the metallic nature of the bases of the alkalies and earths; the second, the method of treating the tests for the compounds of arsenic with oxygen. We quote from the first-mentioned:—

"An examination of the works on chemistry, will show a constant uniformity in ascribing all the credit of discovering the alkaline and earthy metals, to Sir Humphrey Davy; and I am quite willing to do full justice to the memory of that eminent and highly useful philosopher. It does appear to me, however, that injustice has been done to Lavoisier in this matter." "It will not be denied, that a part of the book called the *Elements of Chemistry*, was not written by Lavoisier; but the whole is so entirely devoted to the antiphlogistic system as to give the production a singleness of character, justifying the appellation so uniformly applied to it."

"In a note at the foot of page 213, the translator says, 'there are some experiments related in the Transactions of the Turin Academy, which give reason for suspecting that soda is a modification of magnesia; this latter substance, according to the experiments detailed by Baron Born, seems to be a metallic *oxyde*. From analogy, we may presume, that *potash* is also a metallic substance, in some hitherto unknown state of combination. We shall thus exclude all the alkalies from the class of simple, elementary substances.'"

"In page 217, Lavoisier thus writes"—

"It is extremely probable that barytes, which we have just arranged with the earths, is an oxyde; for in many experiments, it exhibits properties really approaching to those of metallic bodies. It is even possible, that all the substances we now call earths, may be only metallic oxydes, irreducible by any hitherto known process."

"A section is added by the translator, on the *metallic nature of the earths*, in which, after detailing the processes by which the metallic bases of lime, barytes, and magnesia, were repeatedly obtained; he uses the following language"—

"Should they be *eventually confirmed* by rigorous examination, a new light will be thrown on several of the most difficult parts of chemistry, in exact conformity with the predictions of Lavoisier, who supposes that those substances which have long been considered as primitive earths, are only metallic oxydes, and that their reduction has hitherto been prevented by the attraction which subsists between them and oxygen, being stronger than that between oxygen and carbon."

"To this account, Baron Born adds, that"—

"He expects soon to learn, that the silicious and argillaceous earths are likewise metallic oxydes; and that in this case, the whole class of earths and stones will disappear from the mineral kingdom."

We have not spoken of the peculiar adaptation of this work to students of medicine, since it contains about as much as other general treatises, if not more, that is directly interesting to them; in this point there might we apprehend be considerable improvements in works intended specially for the student of medicine.

B.

XXII. *Intorno alla Natura del Miasma Cholèroso Asiatico congetture di* B. M. Trasmesse alla Società Medico-Chirurgica di Berlino. Lucca, 1832. *Conjecture relative to the Nature of the Miasma productive of Asiatic Cholera.* By B. M. &c.

The author of the little work whose title is prefixed to this article, Professor Mojon, of Genoa, occupies very deservedly a conspicuous station among the members of the medical profession in Italy, both as a profound and erudite pathologist and physiologist, and as a skilful physician. His reputation is not limited to his native country, but has extended abroad, where he is known as the author of many valuable publications, some of the principal of which have been translated into various languages, and widely circulated in France, Germany, and England. Occupying, during the dominion of Italy by the French, a professorial chair in the medical department of the University of Genoa, which chair, it may be proper to remark, he gained by concours among twenty-four competitors, he shared, soon after the transfer of Genoa to the Sardinian government, the fate of many of his most distinguished colleagues, and was dismissed on account of his political sentiments, from the school of which he was considered one of the brightest ornaments. But this event, far from lowering him in the estimation of his countrymen, the majority of whom are, as may be presumed, hostile to the present order of things, or abating his zeal for scientific pursuits, tended to render him perhaps more popular than he was before, and, as we may judge from the various physiological and practical works, with which he has enriched the medical literature of his country, to stimulate him to further efforts.

The last, and not the least interesting of those publications, is, we believe, the one at present before us. This essay was written, as we are informed, with a view to its being published at Genoa. Meeting, however, with opposition on the part of government, and not feeling disposed to modify his views, or expunge passages which were thought obnoxious, the author changed his plan, and addressed his manuscript to the Medico-Chirurgical Society of Berlin. A short time after, a copy of the work found its way to Lucca, and was there printed and published, without, however, as we learn from the editor himself, the sanction of the author.

This unwillingness on the part of the government of Genoa to allow the publication of an essay on a purely scientific subject, would, perhaps, have been calculated to surprise us, were we not fully aware of the difficulties which authors encounter in countries deprived of the liberty of the press, and of the ridiculous height to which opposition to the manifestation of independence of sentiment is carried in the territories of his Sardinian majesty. A very careful